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DISCUSSION AND CORRESPONDENCE.

THE LENGTH OF A CURVED LINE.

I SHOULD be very sorry to have anyone interpret my remarks in a recent number of SCIENCE (see page 533) as imputing ignorance of fundamental principles to so distinguished a geometer as Prof. Halsted. In saying that Prof. Halsted 'appears to believe' that he has given a logically complete discussion, my meaning was that he so appears to the unassisted reader of his 'Elements of Geometry.' My criticism was directed at the book rather than at the man. Further, as he says in his reply on page 656 of SCIENCE, the criticism is not applicable to his more recent work, 'Elementary Synthetic Geometry.'

In my opinion, it is not possible to discuss, in an elementary manner, propositions relating to the magnitude of curved lines until after the introduction of the following postulate: *The magnitude of a curved line is the limit toward which a broken line made up of consecutive chords of that curved line approaches, when the number of chords is increased in such a manner that the chords are all diminished without limit.* After the introduction of this postulate it is possible to compare the magnitude of a curved line with that of a straight line.

To turn again to Prof. Halsted's 'Elements of Geometry,' not only was it an error of logic to attempt to demonstrate without this postulate, or its equivalent, that a straight line is the shortest line joining two fixed points; but it was an error of the same sort to introduce, on pages 162-165 of that work, propositions relating to isoperimetric figures, which from their very nature depend on a comparison of non-congruent lines.

It seems worth while to insist upon the points made in this note and in my preceding note, because they relate to subjects treated in almost every American text-book of geometry; but in none, so far at least as the writer is aware, has a thoroughly satisfactory treatment been given.

In the very recent text-book of Beman and Smith, of which the writer has expressed a high opinion (See SCIENCE, this volume, page 203), the following appears on page 187:

"POSTULATE OF LIMITS. The circle and its

circumference are the respective limits which the inscribed and circumscribed regular polygons and their perimeters approach if the number of their sides increases indefinitely.

"This statement is so evident that a proof is not considered necessary. Like valid proofs of many fundamental principles, it is too difficult for an elementary text-book."

The statement consists of two parts, one relating to superficial magnitude, the other to linear magnitude. The former is capable of simple proof. The circle is greater than any inscribed polygon, and any circumscribed polygon is greater than the circle; by the axiom, *the whole is greater than any of its parts*. Proofs based upon these considerations are older than the text of Euclid. The second part of the statement is a 'postulate' in a strict sense. It cannot be proved at all except from equivalent assumptions.

THOMAS S. FISKE.

OCTOBER 31, 1896.

ON CRITICISMS OF ORGANIC SELECTION.

A LONG absence in Europe has prevented my seeing several criticisms of my papers in this JOURNAL, until very recently; and although the issues may now be forgotten by the critics as well as by the readers of SCIENCE, I venture to write a few lines, if only to express my thanks for the kindly words which have aided me to see where the articles were not clear.

First, I may say that I have published, in the *American Naturalist* (June and July, 1896), a paper of some length under the title 'A New Factor in Evolution,' gathering the positions of the SCIENCE articles into a single sketch, thus carrying out, to a degree, the suggestion made by Prof. Wesley Mills in SCIENCE, May 22 (a suggestion which, however, I did not see until my return in September). Condensed summaries of the two main positions involved in the doctrine of Organic Selection (which I ventured to call a 'new factor') were quoted in this JOURNAL for July 31, p. 139, and I need not stop to requote them.

I am glad to know, both from Prof. Mills' article in SCIENCE, May 22d, and also from a personal letter from him, that he accepts the class of facts which I have emphasized, and admits their importance (having himself before

pointed out the imperfection of instinct)*; the point of difference between us being in their interpretation with reference to the inheritance of acquired characters. I hope the charge of obscurity which he brought against my SCIENCE articles holds to a less degree of the fuller presentation of the case against Lamarckism in the papers in the *Naturalist*. I may express the wish—in the way of a friendly suggestion of a reciprocal kind to Prof. Mills—that he take up the arguments which I have advanced to show that the Lamarckian view of heredity is not entitled to the exclusive use of the principle of use and disuse, but that evolution may profit by the adaptations of individual creatures without the inheritance of acquired characters, through what I have called Organic Selection, and show why they do not apply.

As to the 'newness' of the general view which I have published, that is a matter of so little importance that I refer to it only to disavow having made untoward claims. Of course, to us all 'newness' is nothing compared with 'trueness.' As to the working of so-called Social Heredity, I am not aware that I called the position new, *i. e.*, that social influences do aid the individual in his development and enable him to keep alive. This had been taught by Wallace, and later was signalized—as a note on my papers points out in *Nature*—by Ritchie and by Weismann. What I thought was new about Social Heredity was the name, which seemed to me appropriate for reasons given in the *Naturalist* articles, and also the use made of it to illustrate the broader principle of Organic Selection—which latter principle I did and do still think to be new. A word in regard to it.

If we give up altogether the principle of modification by use and disuse, and the possibility of new adaptations in a creature's own lifetime, we must go back to the strictest Preformism. But to say that such new adaptations influence phylogenetic evolution only in case they are inherited, is to go over to the theory of Epigenesis. Now what I hold is that these individual adaptations are real (*vs.* Preformism), that they are not inherited (*vs.* Epigenesis), and yet that they influ-

ence evolution. These adaptations keep certain creatures alive, so put a premium on the variations which they represent, so 'determine' the direction of variation, and give the phylum time to perfect as congenital the same functions which were thus at first only private adaptations. Thus the same result may have come about in many cases as if the Lamarckian view of heredity were true. A case of special importance of this is seen in *intelligent adaptations*, and one of the most interesting fields of intelligent adaptation as that of *social cooperation*.* The general principle, therefore, that *new adaptations effected by the individual may set the direction of evolution without the inheritance of acquired characters* is what I considered new and called Organic Selection (also for reasons set out in the *Naturalist* articles).

Prof. Cattell, writing with thorough appreciation of the principle (in *The Psychological Review*, September, 1896, p. 572), cites Darwin's doctrine of Sexual Selection as a case from the literature. This case also occurred to me this summer. Apart altogether from the truth or falsity of Sexual Selection, the use which Darwin made of it was directly in the way of what it seemed well to me to call Organic Selection. Sexual Selection would be, if proved, a particular and special case of Organic Selection. But Darwin, as I think—subject to correction by those more familiar with the literature—found the importance of Sexual Selection in the fact that it took effect directly in the pairing of mates and so influenced posterity. I do not know that Darwin advanced the general truth that all personal adaptations which were of 'selective value'—*i. e.*, which were useful enough to enable a creature to escape with his life—would bring about indirectly the sort of effect upon pairing that Sexual Selection would. But whether he did or not, if that be true, then evidently the special case of Sexual Selection does not cover the whole influence, and there is the same reason for giving the whole influence or 'factor' a name that Darwin had for giv-

* The phrase 'half-congenital,' referred to by Profs. Mills and Bumpus, was used as expressive rather than as a suggestion in terminology!

* These are the two main cases dealt with in my SCIENCE articles, and to my mind (speaking for no one else) the main interest attaching to the imperfection of instinct, discussed lately by various writers in these pages, is that it shows this 'factor' at work.

ing a special name to the particular case of Sexual Selection.

In short, does not the formulation of any positive influence which regulates the operation of Natural Selection really indicate a 'factor' in the whole evolution movement? Darwin formulated Sexual Selection as such a factor. Wallace's 'recognition-mark' theory of the origin of bright plumage in male birds is another such formulation. Organic Selection formulates the general factor which both these positions—and possibly others—illustrate; 'newness' in any other sense I am not disposed to maintain for it.

Darwin's personal use of the principle of Sexual Selection, I may add, seemed to require a very high psychological development on the part of the choosing mate, the female; but the way that the principle may be generalized—although still with reference to the special case of mating—may be seen in the very interesting suggestions of Groos (*Die Spiele der Thiere*, pp. 230 ff).

More than one of my critics have spoken of the relation of Organic Selection to Natural Selection. It is discussed at some length in the *Naturalist* article (July, pp. 549 ff). Prof. Cattell says: "It is the essence of Natural Selection that under changed environment those individuals will survive who can best adapt themselves to it." Certainly it is. But I think that the advocates of Natural Selection have considered as useless or unimportant in evolution those adaptations of individuals which were not adequately represented in the *congenital equipment* of the individual. Certainly the tendency, at least, of the Neo-Darwinians has been to deny the influence of the principle of use and disuse on evolution—to consider it altogether a part of the machinery of Lamarckism.* *The influence of new adaptations, however, in determining the limits of variation in subsequent generations without appealing to the inheritance of acquired characters*; that (to repeat) is the combination which I considered new, although I should not have had the courage to label it so if certain biologists familiar

*Thus they would say: The intelligence is congenital, but the particular things learned by intelligence, not being inherited, have as such no influence on race development, except as the children also learn to do these things intelligently.

with the history of discussion had not so characterized it.

If Romanes, for example, had thought of this answer to Lamarckism, I cannot conceive that he would still have pressed his argument for the inheritance of acquired characters drawn from the coordinated muscular movements seen in instinct; and in this particular case—the origin of instinct—I think the doctrine of Organic Selection gives a new theory.

So far, however, from opposing Natural Selection appeal is made directly to it. The creature that can adapt itself gets its value only because it is selected, as Natural Selection does all its selecting. Even might we say that the very ability to make personal adaptations may possibly be due to Natural Selection. But I can not go with Prof. Cattell in saying: "If Organic Selection is itself a congenital variation, as Prof. Baldwin indicates [as possible,]* we are still in the *status quo* of chance variations and Natural Selection." Not entirely, I think, since the future variations are narrowed down in their range within certain limits. Say a creature is kept alive and begets young because he can adapt himself intelligently or socially, and say his mate has the same character; then the drift of variations in the next generation will be in the same direction, as Prof. Cattell himself recognizes.† Of course, as far as this point goes, we do 'remain ignorant as to why the individual makes suitable adaptations;' that is quite a different question, involving I think, for adaptations in the sphere of muscular movement, another application of Natural Selection, *i. e.*, to overproduced or excessive movements‡; but we do not remain ignorant as to 'why congenital variations occur in the line of evolution,' admitting that they occur at all. And, of course, we do remain in ignorance as to why 'they [variations] are hereditary;' that again is a matter of the mechanism of heredity.

In connection with this question of 'newness'

*Cf. my *Mental Development*, pp. 172 ff. 204 ff.

†In the illustration he gives of Organic Selection, *i. e.*, of dogs becoming granivorous from feeding on grain during many generations.

‡Criticisms of this hypothesis I can not consider now, but hope to answer them soon in *The Psychological Review*.

—as much as I dislike to dwell upon it—I must refer to another remark by Prof. Cattell. He says that I leave it in doubt whether I mean to say that this principle of Organic Selection was stated in my book on *Mental Development*, and also that he can not tell from his memory of the book. This is a fair question. The principle was suggested in the book, as the following quotations will suffice to show: “It is necessary to consider further how certain reactions of one single organism can be selected so as to adapt the organism better and give it a life history. Let us at the outset call this process ‘organic selection,’ in contrast with the natural selection of whole organisms.” * * * “The facts show that individual organisms do acquire new adaptations in their lifetime, and that is our first problem. If, in solving it, we find a principle which may also serve as a principle of race development, then we may possibly use it against the ‘all-sufficiency of natural selection,’ or in its support” (Pp. 175–6). Then in speaking of the results of the individual’s adaptations on the course of evolution: “This again is exactly the same result as if originally neutral organisms had learned each for itself. * * * The life principle has learned, but with the help of the stimulating environment and natural selection (173).” Again in speaking directly of heredity (p. 205 f): “It [Neo-Darwinism] denies that what an individual experiences in his lifetime, the gains he makes in his adaptations to his surroundings, can be transmitted to his sons. This theory, it is evident, can be held on the view of development sketched above, for granted the learning of new movements in the way which I have called ‘organic selection’ * * yet the ability to do it may be a congenital variation. * * * And all the later acquirements of individual organisms may likewise be considered only the evidence of additional variations from these earlier variations. So it is only necessary to hold to a view by which variations are cumulative [*i. e.*, the view of Organic Selection] to secure the same results by natural selection as would have been secured by the inheritance of acquired characters from father to son.” (See also p. 206.) I may be allowed, also, in view of the charge of obscurity made by Mr. Cattell—

and the appearance of which comes in part, at least, from the need of condensation—to cite the following sentences from a review of my book in the *London Speaker*. Giving an exposition of the position which the book takes on the subject of heredity, the reviewer says: “If, however, creatures having the ability to make intelligent adaptations which become consolidated into habits (called ‘secondary instincts’) are selected for survival, it is just as if secondary instincts were acquired by actual transmission to offspring of the modifications produced in parents by the exercise of their own intelligence. Psychologists may, therefore, practically speak as if acquired mental characters were really inherited, though what is inherited may be only the ability to acquire them. Such ability, of course, natural selection would accumulate like any other variation.” The passage which this reviewer refers to is in *Mental Development*, p. 207, a passage which was expanded, *apropos* of Romanes’ doctrine of the origin of instinct, in my paper in *SCIENCE*, March 20, 1896.

While suggested in the book, however, it is not enlarged upon, since the section on heredity was written only to show that either of the current views might be held together with the main teaching of the book.

I regret taking so much space for these personal explanations, but the editor of this JOURNAL can spare the space, since it is he who asked the question!

Prof. Cattell also finds obscurity in my view of the place of consciousness in evolution. The obscurities are possibly cleared up somewhat in an article on ‘Consciousness and Evolution’ in the May, 1896, issue of *The Psychological Review*.

J. MARK BALDWIN.

PRINCETON UNIVERSITY,

October 27, 1896.

THE relations of individual adaptations to race evolution will shortly be reviewed in this JOURNAL by Prof. Lloyd Morgan and by Prof. Osborn. I think that the important principle called by prof. Baldwin ‘Organic selection’ is implicit in Darwin’s works, and has been clearly formulated by Prof. Weismann.

J. McKEEN CATTELL.